Impacts and mass extinctions

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The Alvarez Hypotheses of 1980, that the earth was hit by a large asteroid or comet 65 million years ago, and that the environmental effects stemming from that impact event brought about the K/T mass extinction, are now accepted by a majority of earth scientists. The questions is, then, what next for paleontological extinction research? The following topics seem fruitful: 1) Better research on actual killing mechanisms. 2) Calibrating the "kill curve" of Raup (1990; 1991) relating impact (or crater) size and degree of mass extinction. 3) Understanding selectivity. 4) Improving sampling methodology, and using some of the lessons in sampling developed for K/T (The Signor-Lipps effect, confidence intervals and sampling precision perfected for K/T) on other extinction boundaries.

Introduction

Mass extinctions have traditionally been defied as relatively short periods (usually on the order of one to five million years in length) of greatly elevated extinction. There have been many such episodes during the Phanerozoic Era (the last 530 million years, the time of skeletonized life) although only five can be classified as having been "major", in the sense that more than 50% of all species died out (Figure 1).

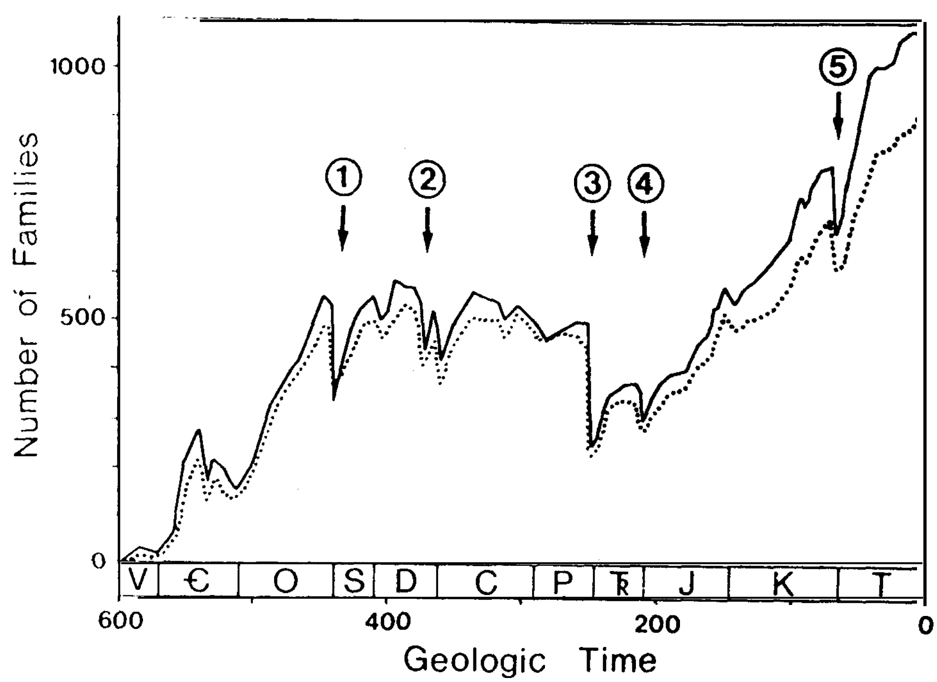

Figure 1. Diversity of marine families through Phanerozoic time, including the five largest mass extinctions. After Sepkoski, 1993

In the last 15 years, there has been a major shift in how we view biological extinctions, particularly with regard to rapid (catastrophic) events of a global extent, and to their potential causes. Sixty percent of all extinctions during the Phanerozoic occurred in short-lived events (termed "pulsed extinctions" by Raup, 1991). Raup concluded that pulsed events resulted in the extinction of more than 5% of all species worldwide living at those times, and took place in time intervals of less than 5 million years.

The causes of mass extinction have been widely debated, with sudden sea level change, climatic cooling or heating, and periods of oceanic being the most widely cited. Recently, however, meteor or comet impact as a cause for mass extinction has been seriously explored, propelled by the hypothesis that the end of the Mesozoic Era was caused by the impact of a large meteor or comet on the earth. (Alvarez et al, 1980).

This suggestion, now known as the Alvarez Impact Hypothesis, was originally controversial, but mineralogical, chemical, and paleontological data gathered over the past decade have confirmed that a large comet impacted the earth approximately 65 million years ago, and that, at the same time, more than half of the species then on earth became extinct rather suddenly. The recent discovery of a large impact crater in the Yucatan region of Mexico (the Chicxulub Crater), now suspected to be multi-ring impact basin as large as 300km in diameter (Sharpton et al, 1994) has virtually swept away remaining opposition to the hypothesis that the end of the Mesozoic Era was brought about by the impact of one of the largest extraterrestrial bodies to crash into the earth since lithosphere formation. Yet this body was by no means the only such large object to have ever hit the earth or perhaps cause mass extinction; many astronomers and earth scientists suspect that other large body impacts may have caused mass extinctions at other times in the past, including the middle Devonian, end-Triassic, and end-Eocene extinctions (Raup, 1990).

There is still some paleontological opposition to the contention that the K/T mass extinction was caused exclusively, or even mainly by an impact, particularly among vertebrate paleontologists (see Rigby et al, 1994), but also among a few invertebrate paleontologists (Zinsmeister and Feldmann, 1994; Keller, 1994). Yet among paleontologists, opposition to Alvarez Part 1 (there was an impact) is now virtually nonexistent, while opposition to Part 2, (it caused an extinction) has been stronger, but now seems to be centering on the concept that the extinction was limited to the tropics (Keller, 1994; Zinsmeister and Feldmann, 1994). Others dispute this, however, and believe that the end Cretaceous mass extinction was equally disastrous throughout the world (Raup and Jablonski, 1994; Marshall, 1994).

There is still much to learn about the timing, details, and mechanisms involved in the K/T catastrophe; there are probably decades of research necessary to flesh out the picture (the possibility of latitudinal gradients in the extinction, selectivity among victims and survivors, and perhaps the presence of high latitude refuges may be the most poorly known aspects). Nevertheless, like plate tectonic theory in the late 1960's, we are early in the era of a new paradigm: a new, grand picture has emerged, but many details remain unknown.

Some of the major lessons from the K/T debate are as follows: 1) Models or mechanisms of lethality are still in their infancy, and are, as yet, inadequate to explain patterns of selective survival and extinction. 2) Impacts can cause extinction. 3) Usually there is more than one killing mechanism going on in any mass extinction - even the K/T event 4) The fossil record literally interpreted at extinction boundaries can be very misleading. 5) All extinctions should be considered as rapid unless proven otherwise. 6) Breakthroughs will be the result of multidisciplinary efforts, often involving paleontology, sedimentology, geochemistry, and atmospheric science.

There are, of course, numerous directions that research into mass extinctions can and will take in the post K/T era. Four that I consider important are described below.

Killing mechanisms

One of the most frustrating aspects of paleontological research on mass extinctions is that models about the actual killing mechanisms are so poorly constrained. Only paleontology can arrive at an accurate list of victims. But rarely can the actual killing mechanism be deduced from literal interpretation of the fossil record. This scenario is at its most exasperating in trying to explain the most consequential extinction of all, the-Permian catastrophe. The accepted mechanisms are climate change following the formation of Gondwanaland, and marine regression as the northern and southern continents came together, approximately 250 million years ago. These are surely related - somehow - to the catastrophic, end-Permian mass extinction. But can they account for extinction of 90% of all species, according to Raup (1979)? And not just in the sea, but on land as well?

One of the most perceptive comments about causes of extinction came not from a paleontologist, but from a petrologist. At a recent meeting, V. Sharpton (personal communication, 1994) said: "Mass extinctions are caused by changes in the global atmosphere inventory". The cause of the atmospheric gas changes (which may be changes in volume or relative constituents of the atmosphere), of course, can be caused by many things: asteroid or comet impact, degassing during flood basalt extrusion, sea level change, etc. But the actual *killing* is brought about through changes in the makeup and behavior of the atmosphere (such as temperature and circulation patterns) that are dictated by properties of the atmosphere. At least for K/T, those few scientists worrying about kill mechanisms tend to agree. The Chicxulub comet, after all, *directly* killed very little. Its effect on the composition of the atmosphere, however, was probably far more lethal. The studies of Pope et al, 1994, on temperature change, Sigurdsson et al, 1992, 1994; and Dhondt et al, 1994, on the effects of sulfur, and Covey et al, 1994, who looked at the global climatic effects of atmospheric dust produced by the impact of a large (10 km) asteroid or comet, all suggest that killing mechanisms were atmospherically forced. The latter study may be particularly important: the Covey et al study suggested that fine dust generated by impact, into *either* an oceanic or continental target area would produce long-term (on the order of months) cooling of land areas, and wholesale changes in the hydrological cycle. The latter effect - the rapid change of rainfall precipitation patterns - may have been particularly lethal to plant ecosystems.

Clearly, we are just beginning to understand killing mechanism. The fine synthetic work of Toon et al (this conference), examining the additive effects of various mechanisms, seems a fruitful new approach.

Calibrating the kill curve: impact size and mass extinction

The killing potential of an impact event must be related to many variables. Clearly, however, the size of the incoming body is among the most important in determining extinction rate. Raup (1990) has followed this line of argument, and proposed a "Kill curve", relating impactor body size (and crater diameter) to percentage of biota expected to be eliminated. This leads to the following questions:

1. Are any other mass or pulsed extinctions (in addition to K/T) caused by impact?
2. Besides size, what determines the killing potential of an impact event?.
3. Can the "kill curves" as proposed by Raup be validated from newly acquired data? Specifically, are the curves monatomic, (as postulated by Rap, 1990), in that increasing crater size and hence impactor body size leads to increased rates of extinctions, or is there some threshold (in size of crater or impacting body involved), such that some critical size leads to extinction, but below that size no, or minimal extinction takes place?.
4. Do impacting bodies kill in the same way regardless of size, or is killing mechanism related to body size? In other words, does size alone alter the kill mechanism, or is killing effect always the same, but increases or decreases in some regularly scaled manner?

In the heady days following the Alvarez hypothesis, some investigators thought that a general synthetic model linking most or all mass extinctions to impact would emerge. The hypothesis that mass extinctions show a 26 million year periodicity (Raup and Sepkoski, 1984) is inherently based on this assumption. By the latter part of the 1980s, however , it became clear that rather than being a typical mass extinction (all caused by an impact), the K/T catastrophe appeared to have been a unique event. Gradually, as boundary after non-K/T boundary did *not* yield evidence similar to that routinely found at K/T boundary sections around the globe, it was argued that K/T was perhaps the *only* one of the big five mass extinctions (Ordovician, Devonian, end-Permian, end-Triassic, end-Cretaceous) to have been caused by impact. Even periodicity lost its luster, since no single cause that could produce periodic extinctions could be discerned.

By the early to mid-1990's, the pendulum began to swing back once again, and continues to do so. Rampino and Haggerty (1995) have recently summarized major extinction boundaries yielding evidence of impact; they report on the findings of elevated iridium from two Precambrian/Cambrian boundaries, three Ordovician/Silurian, three Frasnian/Famennian, one Mississippian/Pennsylvanian, thirteen Permian/Triassic, one Triassic/Jurassic, one Callovian/Oxfordian, one Jurassic/Cretaceous, one Cenomanian/Turonian, more than 100 K/T, "widespread" (their term) late Eocene, one Middle Miocene, and one Pliocene locality. They argue that numerous extinction boundaries *do* indeed yield geologic and geochemical evidence of impact. It is clear, however, that the K/T impact is in a class by itself due to its extremely elevated iridium values, shocked quartz, spherules, and ubiquity of sections showing these traits. It may have been caused by the most energetic impact event certainly of the Phanerozoic, and perhaps for much of earth history (Sharpton et al, 1994).

One of the most obvious, yet powerful generalities to emerge from this period was Raup's concept that impact events would fall on a kill curve; the bigger the impact event, the greater the percentage of global fauna going extinct (Raup, 1990, 1991). As with much of Raup's work, this idea is both simple and powerful (Figure 2). Unfortunately, the curve itself is entirely theoretical, as very few large craters have been sufficiently well-dated or studied to be confidently tied to mass extinction events. Virtually the *only* crater known with any degree of confidence to be associated with mass extinction is Chicxulub. Rampino and Haggerty (1995) attempted to test the validity of Raup's curve by adding in information about three additional craters: Puzech-Katunki, 80 km, Triassic; Popigai, 100km; Tertiary; Manicouagan, 100km; end-Triassic. These additional points seemed to fall within the envelope of error as hypothesized by Raup (Figure 2).

The kill curve concept is one of the most powerful to emerge from the entire extinction debate; one of the prime goals of future extinction research should be tests of its validity. The kill curve concept is probably valid, but one single curve may be insufficient to model the effect of impact and extinction. Many variables must come into play, including factors associated with the incoming body (its size, composition, angle of impact and velocity; Schultz, 1994), as well as the nature of the impact area (the target area). The geology of the target region may have profound implications on the degree of kill. Moreover, not only the *geology* of the impact site, but its *geography* as well may play an important part: an impact in a low latitude site will surely have entirely different consequences from a similar body hitting with similar angle and speed at a high latitude site, since the distribution of lethality across the globe may be produced by atmospheric circulation patterns. Finally, the nature of both the biota and the atmosphere at the time of impact are surely important: An impact in a highly diverse world of specialists will surely have different effects than one impacting a low diversity world of generalists, just as impact into a Greenhouse world may have different effects than one where greenhouse gas inventory, or, say, oxygen content, is lower than that today.

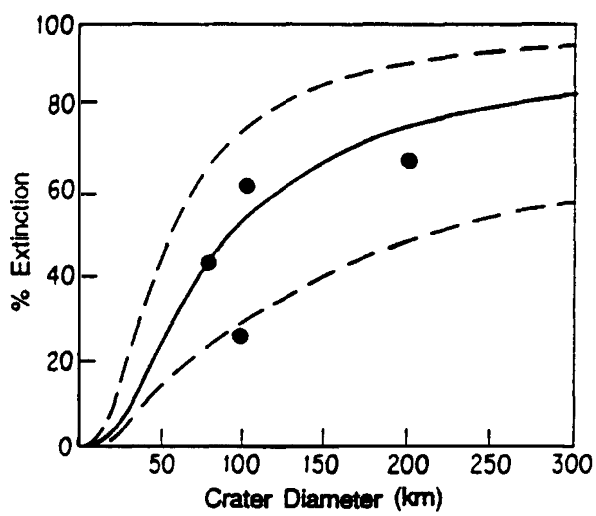

Figure 2. The Kill Curve of Raup, 1990, with data points for Puzech-Katunki, Popigai, Manicouagan, and Chicxulub placed on the curve by Rampino and Haggerty (1995). It should be stressed that at this time the extinction percentages for all but Chicxulub are speculative.

The way which impacts have been linked to mass extinction has proceeded in a less than ideal fashion. In the past, mass extinction horizons have been identified in the stratigraphic record based on paleontological data, and then evidence for impact (iridium, spherules, shocked quartz, etc.) has been searched for *at that horizon*. If physical evidence for impact is found, the last step in the process is identifying a suitable crater. This was the methodology in K/T, essentially. It would be more reasonable *start* with the crater. This, of course, requires that the crater be precisely dated, a major flaw in the past. But when large impact craters are dated with precision, stratigraphic horizons of those ages can be searched for extinctions and physical evidence of impact. This methodology is exactly opposite of that employed to date (Ward and Sharpton, 1994).

Selectivity

Faunal selectivity is surely one of the most powerful clues about mass extinction. Which species survived, and which perished, is partly related to pure chance, surely, but must also be ascribed to effects and killing mechanisms as well. This rich source of information has only begun to be tapped for the K/T catastrophe, and even less so for other extinction boundaries. Nevertheless the information to date is fascinating (see Jablonski, in press, for a review). Among more recent work, Kaiho's (1992) study showing relative rates of planktonic and benthonic foraminiferal extinctions during K/T, as well as Markwick's (1994) use of biological data on selectivity are novel. The former gives us a rough bathymeter of lethality, while the latter shows that the terminal Cretaceous extinctions were not primarily caused by climate change. Further analysis of selectivity is surely one of the most fruitful avenues for future research.

Breakthroughs from K/T should be exported to other mass extinction boundaries

The K/T controversy has made paleontology far more rigorous. A whole new way of looking at extinction boundaries has not only improved the proximal research, but has greatly benefited biostratigraphy as a whole (Ward, 1990). The most notable breakthroughs have been the recognition of the Signor-Lipps effect (sudden extinctions will usually look gradual; Signor and Lipps, 1982), the realization that stratigraphic hiatus can obscure true stratigraphic pattern (gradual extinction can look sudden), and the use of confidence intervals (Marshall, 1990) in studying boundaries.

The case perhaps most in need of applications learned from K/T (and certainly where we stand to learn the greatest information), is the Permo-Triassic extinction. Being much older than K/T, and having a marine regression right in its midst (which at best, reduces information, and could certainly be involved in the mechanisms of the extinction itself), it is clear that we are still looking for a reasonable model. Erwin (1993) has proposed his "Murder on the Orient Express" hypothesis (multiple causes); Hallam (1994) invokes anoxia, while Rampino and Haggerty, (in press) suggest impact. The main problem is the paucity of sections. The best place to study the P/T extinction may be in the Karoo region of South Africa. The P/T boundary there is exposed over hundreds to thousands of miles, and vertebrate material is quite common. I had the opportunity to study these sections (Ward, 1994), and came to the conclusion that the extinction, at least among land vertebrates, was very rapid. In fact, the situation in the Karoo beds is similar to, and should be studied in ways analogous to the Hell Creek dinosaur beds (Sheehan, et al, 1991). By making carefully documented stratigraphic collections of plant and vertebrate fossils from these sections, and then looking at the possibility of Signor-Lipps effect as well as establishing confidence intervals, a straightforward field program could glean, in short order, a great deal of powerful insight into the rapidity, and perhaps, the cause, of the Permo-Triassic extinction.

Post script: Are we currently in a mass extinction?

Most definitions of mass extinction suggest that a typical episode involved the extinction of around half the species on earth in a million years or less. Has the current day biota entered such a phase?

I recently wrote a book (Ward, 1994) suggesting that such is the case. Yet proving this contention is exceedingly difficult. The real problem, of course, is the fact that we only have the *faintest* idea about how many species currently are extant on earth, and thus cannot tell what percentage of total species biodiversity is actually disappearing per year. Approximately 1.6 million species have been defined to date, but most taxonomists agree that there are far more, especially among tropical insects. Peter Raven of the Missouri

Botanical Garden estimates that there are a *minimum* of ten million species, while E. O. Wilson (1993) suggested that there may be a many as *30 million* species. Yet, if we have such a poor handle on how many there are, how can we arrive at reasonable estimate about how may are going extinct? It is my contention that the current mass extinction is already well underway. This hypothesis can only be tested by paleontology.

There is probably no more politically charged arena than trying to discuss whether we are, or are not in the midst of a current mass extinction. Certain facts are inescapable. Barring some unforeseen population collapse, nuclear Armageddon, or large body impact, human population will double to more than 11 billion people by the end the next century; humans are large animals and co-opt resources; humans cause extinctions. At what point (if ever) will enough species be killed off to qualify us as having the dubious distinction of being in a recognized mass extinction event? Certainly, a significant proportion of large mammals have already gone extinct in all continents save Africa in the last 40,000 years (over 50 genera in North America alone in the last 15,000 years). And certainly the rampant reduction of forest cover in the world, especially in the tropical rain forests, is leading to species extinction at some currently unknown rate. The most extreme estimate I have heard comes from Peter Raven: he suggested (oral. comm., the University of Washington, 1995) that 60% of all species on earth will be extinct by 2300 AD. As current world biodiversity seems to be far higher than anytime in the past, this will, if it comes to pass, make the current crisis the most devastating (in absolute numbers of species going extinct) mass extinction of all time.

The current crisis in biodiversity (if it exists) thus competes with mitigation efforts for asteroid or comet defense. How much should we spend on detection and mitigation of the comet threat, versus saving rain forests by increasing biological reserves? If a finite amount of money is available, must we choose between asteroid defense or species endangered by earth-bound causes? What are the relative risks to *our* species of dwindling biodiversity? Is it more important to save human infrastructure than non-human biodiversity? And in a far more barren world, a millennium hence, will we even care about saving ourselves from a newly detected, incoming comet? We are in need of a thoughtful, global debate over these questions.

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